

September 16, 2018

Dear Editors:

First of all, we greatly appreciate the critical, helpful, and highly detailed comments from the reviewers and the editor. In general, we agree with the majority of reviewer comments and have endeavored to address all of them in this revision. In the small number of instances where we have not made changes exactly as suggested, we have provided a detailed explanation of our reasoning and motivation.

In the rest of this document, we first summarize the major changes we made in this revision. We then provide our feedbacks in **red** below each comment item in the original review. (The item enclosed in parentheses is a summary of some detailed comments)

Major Changes:

Feedbacks in Detail: (the feedbacks are in **red** under the action items in the original review)

Reviewer #1:

1. I found the discussion about the user-involvement in the performance-accuracy trade-off too superficial. This aspect is important and has an impact on analysts' trust.
We added discussion of performance-accuracy trade-off in Section ??. **Related improvements include experimental evaluation in Section ???.**
2. I found it sometimes hard to figure out what is a novel approach (new algorithm) and what is just a description of something that has been done before/incremental work (e.g., the descriptions in Sections 3.2.2 and 3.2.3)
3. generally the field of progressive analytics should be mentioned, examples [5, 6] and esp. [7]
Added description of progressive analytics in Related Workd (???.??).
4. Detailed comments: ...
Based on the comments, all problems in grammar, layout and other aspects have been solved or improved.

Reviewer #2:

1. In Section 4.2. the author should add some labels to Figure 6, showing the different sections of the interface described at the beginning of Section 4.2.

2. The given scenarios give only a short overview of the functionalities. To better understand, the visualizations, more Figures could be valuable.

Reviewer #3:

1. ...However, the evaluation provides little support for the performance claims made in the paper, making it difficult to evaluate the contribution overall.
2. ...However, there is little discussion of the explicit differences between the RSATree technique and existing approaches to concretely ground the performance claims made in the paper.
3. The paper mentions that the approach exchanges precision for speed. While the resulting evaluation and discussion offers some insight into that trade-off, existing systems for progressive visual analytics often leverage uncertainty representations that allow systems to communicate approximately how correct the computed value is likely to be. Is that possible with the presented technique? If not, how might systems allow analysts to reason about approximation errors caused by the approach (and are the errors likely to be sufficiently large that such reasoning is necessary).
4. The integration of user control is a nice touch. However, given the abstractness of the parameter space, can analysts reasonably tune the accessible parameters? Are the reasonable defaults that could be (or should be) provided?
5. The claim that the scale alignment scheme implemented “in practice... eliminates about 90% of interpolation computation” has no explicit evidence to suggest where this estimate arises from.
6. The evaluation measures time and error performance on a single dataset. However, several of the claims about the technique revolve around space. Space performance is discussed in Table 2, but not in the text. Were there any notable performance improvements with the benchmark test relative to the space?
7. Further, the evaluation only compares versions of the RSATree approach and tests performance using only a single dataset. The evaluation does not support the claims made about the contributions of the technique. To do so, the evaluation should consider a broader collection of datasets and

data scenarios. A comparison should be provided benchmarking these values against other state-of-the-art approaches to validate the performance claims made in the manuscript.

Added evaluations that compare our system with other state-of-the-art approaches. See more details in the Major Changes (#??).

8. ... This discussion of trade-offs should be expanded to identify situations where the RSATree approach would be explicitly beneficial.

We added discussion of performance-accuracy trade-off in Section ???. Related improvements include experimental evaluation in Section ???.

Reviewer #4:

1. A demo version is not available, unfortunately. In case of acceptance I encourage the authors to make the implementation available as open source.

2. ... However, I am missing a thorough comparison with existing state of the art techniques.

Added evaluations that compare our system with other state-of-the-art approaches. See more details in the Major Changes (#??).

3. Minor issues ...

Based on the comments, all problems in writing and grammar have been solved or improved.

Reviewer #5:

1. A central way in which this current submission falls short is the lack of comparison with existing, available alternatives.

2. (Dataset used are not big enough.)

3. If the goal is to allow a clear comparison of this technique to previous and future proposals, the paper must do a much better job at describing how to get to those datasets.

4. Table 2 should do a much clearer job at describing which dimensions can be used for filtering, which dimensions can be used for aggregation, and which queries take longer (or shorter) to execute.

5. What are the tradeoffs? If the # dimensions increases, does query time increase? By how much? What about preprocessing time?

Yours Faithfully,

Honghui Mei